Integrated Regional Wetland Monitoring−II

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Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0166: Integrated Regional Wetland Monitoring−II

Final Panel Rating
superior

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

superior
a large, complex set of projects closely linked together

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

superior
integrated in a quantitative way; well laid out, with completion times defined; analyses and intepretations well defined, with relationships quantitatively dove−tailed

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are

there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

superior
well laid out plan; adequate time and resources allocated for each task; team is knowledgable and experienced in dealing with such a large and complex study; most of team has worked together in the past on similar programs

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

superior
a dream team−−well balanced, possessing complementary skills and knowledge

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

superior
there is a clear plan, based on years of experience working on bay−delta studies, and with communication with shareholders and the public

Additional Comments:

Collaboration Panel (Discussion) Review

Primary reviewer acknowledged that proposal is a large, complex project and the second phase of a program funded earlier. He was confident of individual scientists and their accomplishments and past experience and rated it Superior in

all categories.

Secondary reviewer differed from the Primary in scoring the proposal and rated it Above Average in Interdependence and Integration and Project Management categories. Second reviewer was more critical; because of the sheer size of the project, the Reviewer expected to find a detailed discussion of the process for decision−making and a problem−solving.

Considering the standards applied in the panel discussion, Secondary reviewer believes that this project should be held to higher standards given number of institutions involved (10). There is no discussion of barriers and complexities, and the applicants did not pull on past experience.

It was noted that the Selection panel will evaluate the past performance of ongoing proposals and refined methodology and models based on earlier experience and results.

After deliberation, both reviewers rated the proposal Superior, with the only concern that a project of this magnitude has not included a discription of decision−making process nor addressed how to identify and resolve complexities.

Technical Synthesis Panel Review

Proposal Title

#0166: Integrated Regional Wetland Monitoring−II

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The PIs have the luxury of already having some baseline data from which to design their study, including site information. Collection and analyses methods seem appropriate, with the exception of the vegetative component. Measuring porewater salinity is critical, but sulfate that enters pore space can have a short duration or long one. This measure is critical to growth and survival of many species of oligohaline plant. Vascular plants on the marsh are one of the critical components in their model and measures of salinity in pore water may not be adequate to explain their growth and/or survival.

Additional Comments:

The PIs have the luxury of already having some baseline data from which to design their study, including site information. Collection and analyses methods seem appropriate, with the exception of the vegetative component. Measuring porewater salinity is critical, but sulfate that enters pore space can have a short duration or long one. This measure is critical to growth and survival of many species of oligohaline plant. Vascular plants on the marsh are one of the critical components in their model and measures of salinity in pore

water may not be adequate to explain their growth and/or survival.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

This proposed research addresses very important and timely questions, and the team has the capability to perform the research. However, the proposal lacked some important details, including its relationship to prior research by the investigators. Similarly, the proposal lacked clarity regarding integration of the many components, how all of the identified questions will be addressed in the resulting products, and how the products will be produced based on the methods described in the proposal. Consequently, there were significant concerns regarding the technical and scientific value of the products resulting from this proposed research. The size of the project team and the potential for problems of coordination also raised some concerns.

Technical Review #1

proposal title: Integrated Regional Wetland Monitoring−II

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Are the goals, objectives and hypothesis clearly stated and internally consistent? The goals (i.e., Develop overall approach and refine monitoring metrics, Rapid and efficient monitoring methodologies, Develop predictive models, validate and refine predictive models) were clearly stated and internally consistent. Is the idea timely and important? The goals/idea of this proposal is definitely timely and continues to build on our existing knowledge from similar prior project.** **Rating: 2 = Very Good: High quality in nearly all respects**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Is the study justified relative to existing knowledge? Yes, this study is justified based on existing knowledge/information gathered during IRWM−1 project.** **Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the**

	proposed work? Yes, the proposed overarching conceptual model clearly provided overview of the project goals as the base for the proposed work. The proposed conceptual model also was modified based on past experience (i.e., IRWM−I). The authors described in great details the concept and the major deriving forces inn their conceptual model. They have also provided conceptual model(s) describing, in details, several areas of interest such as biological outcomes conceptual models, which included vegetation, breeding and foraging bird, and fish−invertebrate−food web conceptual sub−models. **Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified? Yes, the demonstration project (i.e., IRWM−I) is justified and has a proven track record. However, I would have preferred to see the results for "Predictor Metrics" analysis (i.e., representing the key physical and biological processes analysis (page 11) completed prior to imitating new work. I believe all information needed for such analysis was collected during IRWM−I, but not fully analyzed. Clarification/additional information are also needed for which statistical analysis/methods shall be performed (e.g., page 16 refers to "statistical analysis" does this mean correlation coefficient/R2). And on page 20, the proposal stated that during IRWM−II, four of the original six sites used in IRWM−I will be retained. However, information justifying the selecting of four out of six sites is lacking.** **Ratings: 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**• The approach is well−designed and appropriate to meet the project's objectives, and adequately build upon existing methods and knowledge from phase I (i.e., IRWM−I). • The proposal also is likely to make a significant contribution, add and expand our knowledge−base, and will be useful to decision−makers. • Expands on existing knowledge by consolidating all field data and experience from similar prior project, into one database system, for analysis and make recommendations for restoration and decision makers.** **Ratings: 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**• The proposal described and documented the technical approach and its feasibility. • The proposed scale for this project, as described, is consistent with project objectives and would enable and benefit all local and state database users. • The likelihood of this project success is very high.** **Ratings: 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**• Very well planned and comprehensive: updated methods used, and great ideas went into building the monitoring program. • Need to state the frequency of equipment calibration/maintenance (Task 3.2.1). • Need justification for why Dissolved Oxygen (DO) is not considered (Task 3.2.1.a; page 27) among data collected using a CTD? • I very much like the use of "SETs" (Task 3.2.3.a) to obtain surface elevation measurements (long term integration of erosion and sedimentation). I prefer to use Three SETs, instead of two, per site to collect elevation data. • On page 40 the proposal stated that (Task 7.2.1), water samples will be collected and analyzed for phosphate. I'm just wondering why "Phosphorus" is not considered as part of soil sampling in Task 3.2.6? Is there any justification (publications/previous research results) that indicate phosphorus is not a major nutrient issue in these wetlands/sites? It is well documented in the literature that soil/sediment is a major storage component for phosphorus in a wetland. • There are published methods to estimate emergent macrophyte bio−mass based on non−destructive technique (I could look−up and send this information), which needs to be considered for Task 4.2.3. I'm not so sure what methods were used in IRWM−I (page 30 the last paragraph). Ratings: 2 = Very Good: High quality in nearly all respects**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the

project?

Comments	**• It is likely that products from this project will be of value to decision makers. • The proposed approach/project will lead to information that is useful to resource managers, other decision makers, and/or scientists. For example, the framework of the proposed approach is based on expanding on well−established adopted methodology (i.e., IRWM−I). • Data, reports, and outcome of this project is well organized and accessible through easy to use web−enabled methods; All results will be retrievable on−line. • I would like to see some changes in "product deliverables" for some of the Tasks listed in this proposal. For example, in Task 3.1 (page 26) under deliverable" Validation Site Field Work Plan" the actual comparison report needs to be part of this task deliverable. This report is critical in defining the minimum level of effort required to provide meaningful metrics of the physical system. For Task 3.2 (page 27; Filed Data Collection), a report documenting and summarizing the results, comparing water levels collected at selected sites (two two−month periods) with NOS long−term data, is needed. I suspect that the two month in winter and the two month in summer of water level data collection may be enough for such analysis (at a minimum a complete record of 29 days is needed to calculate tidal constituents). I'm recommending a two three−month periods, instead of two two−month periods, for water level data collection, to insure a meaningful and reliable correlation can be established between local and NOS data. From my own experience conducting similar analysis, the calculated correlation coefficient between local and NOS sites was**

	good for one site (R2 > 0.90) and not so good for the other site (R2 < 0.40). **Rating: 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Additional Comments

Comments	**The concept proposed for this proposal is great and must be considered for funding.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**• Track record of authors in terms of past performance is evident (e.g., IRWM−I). Information provided through the "Personnel" section is well organized, and complete. Based on my evaluation, the combined qualifications of the assembled project team are adequate to successfully complete this project. • The project team is capable to implement the proposed project as their track record indicate with similar previous projects; IRWM−I. • The mix of disciplines among team members is clearly evident, which insures that the required actual experience and resources, to successfully complete this project, are available for the proposed project. • Summary of past experience and performance record of project team members provided, in great detail, evidences of their abilities to complete the work proposed under this project.** **Goals Rating: 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**• Great effort was spent to provide all detailed to account for all budgetary information (e.g., "parking and bridge tolls for meetings, document reproduction, and FedEx/Postage Delivery). • Consistency: The labor section as presented has cost per task per participant hours per task, and hourly rates. While, others (e.g., Task 2: Diana Stralberg = 6.5 months &GIS specialist = 5.0 months) were provided by month, no hourly rates were provided.** **Rating 2 = Very Good: High quality in nearly all respects.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**• The proposal strongest points: o The concept proposed for this proposal is great and must be considered for funding. o The proposal also is likely to make a significant contribution, add and expand our knowledge−base derived from IRWM−I (i.e., expand on existing information), and these contributions will be useful to decision−makers. o Look−ahead methods proposed for public distribution of products (page 24); the use of "Science−in−Action" fact sheet. o Contributions from the proposed approach, and their significance, include, in−depth literature review of existing monitoring methodologies, pros and cons of each method, comparisons of the selected approach vs. previously applied methodologies (i.e., IRWM−I), and well tested predictive models to assist in wetlands restoration.**

	Rating 2 = Very Good: High quality in nearly all respects.
Rating	**very good**

Technical Review #2

proposal title: Integrated Regional Wetland Monitoring−II

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The overarching hypothesis for the proposed work is that landscape setting (including salinity, inundation regime, accretion, etc…) dictate local ecology and ecosystem processes. Based on this hypothesis, the goal of the research is to build empirical predictive models that describe ecological processes and functions using a specific array of indicator metrics (i.e. salinity (predictor) leads to vegetation biomass (indicator).** **The 4 objectives outlined in order to achieve this goal are: 1. develop approach and refine metrics (predictor/indicator metrics) 2. determine efficient monitoring methods 3. develop predictive models (based on initial sites) 4. validate/refine predictive models (using validation sites) The objectives are clearly stated at the outset of the proposal and remain consistent throughout each of the many components of this large, collaborative effort. The proposal is extremely well organized, with each team following a similar descriptive protocol in the individual sections. Taken sequentially, the objectives build upon each other. We are in need of good predictive models that are based on empirical data. The authors' proposition to determine a simple, time− and cost−effective monitoring strategy will be an important contribution to future restoration efforts in this region of the Estuary.**
Rating	

	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**If successful (and I believe it should be) this research study will provide several solid empirical predictive models that will be useful in future restoration efforts. This is a significant contribution and provides justification for the work. The development of efficient monitoring methodologies is also important, and provides further justification. The project builds upon the previous IRWM work (ongoing – only ~1 year of data collected thus far?) and relationships with other local and regional projects.** **An overarching conceptual model is presented for the entire project and 3 separate sub−models are presented for vegetation, birds and fish/inverts. The overall model describes how regional scale physical processes influence landscape setting to determine site scale physical and biological processes, which in turn determines the abundance and diversity of local biota. The 3 sub−models, while somewhat variable visually, describe the various factors contributing to the successful restoration of these components of the ecosystem. The remainder of the project description centers around these ideas.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**For the most part, the approach is well designed (but see concerns below), and if successful will provide a great deal of valuable information for planning and monitoring future restoration efforts. The approach as outlined appears to be feasible. One of the most valuable pieces of this proposed work is the generation of less time- and cost-intensive methods for monitoring restoration progress. Many of the techniques currently used are quite labor intensive and expensive. If this work could generate a series of simple metrics whereby marsh health can be assessed, this will be a large contribution to our knowledge base.** **The proposed task of validating the predictive model using validation sites will also add a great deal of value to both the previous work by this team and to future usefulness of the models.** **A further strength of the proposal is the emphasis on integration across disciplines and scales. This is a weighty task, but could generate a great deal of valuable information. In a quick perusal of the past posters presented by this group (at www.irwm.org), it appears that this type of integration has not yet occurred; perhaps it is too early yet in the IRWM I timeline.** **Concerns about the approach (the specific concerns listed below deal primarily with the areas in which I feel most qualified to comment):** **1. My biggest concern about this work is the fundamental study design. The previous IRWM utilized 6 sites, 4 within the Suisun Bay reach of the Delta and 2 in the Eastern Delta. In this second phase, the authors propose to discontinue the 2 Eastern Delta sites and add 4 additional "validation" sites in the**

	Suisun region. One of the remaining IRWM I sites is a natural reference marsh. The 3 restored sites vary in size from 48 to 532 acres, have different pre−restoration history, variable salinity and flooding regimes (and therefore vegetation, sedimentation dynamics, etc…), and an array of adjacent habitat types. It seems to me that this array of antecedent characteristics may confound any statistical replication between sites and confound the overall study design for between−site metrics. 2. The validation effort will use 4 additional sites, paired with the remaining IRWM I sites. At the time of submission, only one of these sites had been selected. Will there be problems securing access to sites that match up with the current sites? 3. Mudflat productivity will not be measured during the winter months. Microalgal productivity is often higher during the winter months, and may provide an important food source during this period. I would suggest including nutrient and mudflat productivity during this time. 4. Microalgal productivity will only be measured in unvegetated areas. Microalgal productivity within the vegetated areas of salt marshes, particularly California salt marshes, often provides the majority of carbon incorporated into higher trophic levels. Even though this may be a good deal of additional work, I think it might be valuable to measure microalgal production in both unvegetated AND vegetated regions of the marsh. What about macroalgal production/biomass sampling? 5. Will nutrients be monitored within the same channels as CTD sampling? This isn't stated explicitly. 6. Vascular plant productivity is only measured in the low marsh (Spartina is the only species mentioned). Why such detailed measurements in this zone, but not elsewhere? Why not extend these measurements into the high marsh? Is Spartina the only low marsh species, even in the more inland/freshwater marshes? Based on the site description, S. foliosa is only present in one of the ongoing IRWM I sites.
Rating	

	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**For the most part, the approach is fully documented. There are a few missing pieces, as described in the previous comments. Again, my biggest concern is with the potential for pseudoreplication between sites, given the variety of landscape settings and antecedent conditions. The description of how the empirical data will be integrated into the predictive model isn't entirely clear. This aside, the work is technically feasible and should ultimately be successful. This is a large and ambitious project, but it appears to be very well−organized with a solid project management structure in place. This is important to a project of this size. The size of the project is consistent with the intended outcomes, and if properly managed should yield quality results.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The monitoring to be conducted in the marshes appears to be appropriate for the questions asked. Again, my only concern is the potential for little/pseudo replication among sites. This could possibly be avoided by selecting specific and overlapping regions within each site.** **The premise of the project is to interpret the monitoring data and to develop valuable metrics and**

	methodologies. While the proposal also contributes basic monitoring data, it is the integration and analysis of this data that makes this project unique.
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**As I see it, the two major products of this work are the identification of valuable predictor metrics for the evaluation of marsh "health", the creation of efficient monitoring methodologies and the development of an empirically based predictive model. These are all valuable contributions to our existing toolkit. The authors are well-connected within the Estuary, and have taken into consideration other projects being conducted in the region, as well as in the Bay as a whole. The IRWM website has a great deal of useful information already and should be a valuable clearinghouse for project information in the future.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The assembled team of scientists is extremely well qualified to carry out the proposed work. Their**

	collective experience in the Estuary is impressive. I have no concerns whatsoever about the qualifications of the personnel or the infrastructure that they have available to them.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	It appears that the majority of the budgetary expenses are for personnel. For the most part the budget appears reasonable. The very large sum of money requested should be more than adequate for the work proposed.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Overall, this is an very good – excellent proposal. The work is clearly justified and will provide some missing pieces to our ability to monitor and evaluate restoration projects.
Rating	excellent